

wherever plants get free access to the open air above, it was one of my own assertions that they must necessarily obtain carbonic acid in abundance. It seems to me difficult to understand how in a still place, where many plants at once are engaged in deoxidising a compound which only normally forms 0.03 per cent. of the atmosphere, there can always be as much of it left as any of them can possibly want. I do not presume to argue with Prof. Dyer upon the subject; but as far as my own comprehension goes, he has not made this point clear to me.

May I venture also to suggest that perhaps another danger surrounds biology, and especially botany—the danger of becoming too technical and too academic? Now that perfect instruments, immense collections, and a long technical training are necessary in order to do anything in biology by the regular road, does not the science run just a little risk of falling into a groove? And is it not well from this point of view that there should be an outside body of amateurs, who will take occasionally a fresh non-professional view of the subject, handling their own problems in their own way, and publishing their own little guesses or glimpses for what they may be worth? No doubt they will often go demonstrably wrong; no doubt the masters of the science will usually find numerous blunders of detail in their work, and may often see reason to disagree with them altogether; and in that case the amateurs ought to receive their corrections with all humility; but is it not a healthy thing after all that the amateurs should do their best, and try to follow out their own lights to their own conclusions? GRANT ALLEN

### Forms of Leaves

YOU have recently inserted several letters from Mr. Grant Allen on the forms of leaves, a question in which I have myself been working lately. Mr. Grant Allen's letters open up a number of interesting questions, but for the moment I will only refer to his suggestion with reference to the reason why water plants so often have their leaves cut up into fine filaments. He tells us that this is because the proportion of carbonic acid held in solution by water is very small, and that therefore for this amount there is a great competition among the various aquatic plants.

The question has already been asked on what grounds Mr. Allen makes this statement with reference to the proportionate amount of carbonic acid. Without entering on this point, I would, however, venture to suggest that the reason for this tendency in the leaves of water plants is mechanical rather than chemical.

It is, of course, important for all leaves to present a large surface for the purposes of absorption with as little expenditure of material for purposes of support as possible. Now delicate filaments such as those of water plants present a very large area of surface in proportion to their mass. On the other hand, they are unsuited to terrestrial plants, because they are deficient in strength and unable to support themselves in air. Take, for instance, a handful of the submerged leaves of an aquatic ranunculus out of the water, and, as every one knows, the filaments collapse. This seems to me the real reason why this form of leaves is an advantage to water plants. It is perhaps for the same reason that low-growing herbs, which are thus protected from the wind so often have much divided leaves.

April

JOHN LUBBOCK

### The Föhn

MAY I be allowed the space of a few lines to point out a defect in the account of the Föhn, given by Mr. Scott in his recent work on "Meteorology," and quoted in the review of that work which appeared in NATURE, vol. xxvii. p. 575. This phenomenon has been fully and clearly explained by Dr. J. Hann in a paper entitled "Einführung in die Meteorologie der Alpen," published under the auspices of the *D. und O. Alpenverein*. Mr. Scott's account of the Föhn attributes rightly the dryness and the cooling of the wind at high altitudes to expansion; but he appears to entirely overlook the *heating effect due to condensation of moisture* during the ascent of the wind.

From observations made in Switzerland, where the Föhn is chiefly felt, Hann has established the following rule: the Föhn is as many half degrees C. warmer at any place in its descent, than it is at an equal altitude during its previous ascent on the other side, as the place is hundreds of metres below the mountain ridge. This he explains by the fact that compression during the descent of the Föhn reverses the loss of temperature due to rare-

faction during its previous ascent; while the wind brings with it over the mountain ridge the heat gained by the liberation of latent heat in the condensation of moisture. This latter amounts at 15° C. to about half a degree C. for each ascent of 100 metres for saturated air. "Therein," says Hann, "lies the explanation of the heat of the Föhn." A. IRVING

Wellington College, Berks, April 21

### The Zodiacal Light (?)

THE same "peculiar appearance in the western sky" as that described by your correspondent, "J. W. B.," was observed here by me on the same evening, April 6. At 7h. om. G.M.T., or fifteen minutes after sunset, I noticed a bright, golden-coloured column of light, well defined, about 4° in length and slightly more than 1° in width, and inclined towards the south. "J. W. B." says it "rose vertically from near the horizon" at his station, Bath. Here it was decidedly inclined to an angle of about 15° towards the south. At 7h. 20m. no traces of it were visible. I have not seen any similar appearance since.

W. H. ROBINSON

N.B.—In the observer's book this observation is entered as "Bright zodiacal light (?), seen at 7h. om." E. J. STONE  
Radcliffe Observatory, Oxford, April 20

REFERRING to the letter of your correspondent, "J. W. B.," Bath, in your last issue (p. 580), allow me to say that this peculiar ray of brilliant light was seen here by myself and many other people at about 6.40 p.m. on Friday, April 6. The sunset was brilliant and cloudless, but from the horizon to about 25° in height immediately above the spot where the sun had disappeared there appeared a ray of light of great beauty and extreme brilliancy; its centre, a delicate rose colour, graduating to the edges into the purest gold. This single ray was perpendicular, and appeared to be little, if at all affected, in its brilliancy by the approaching dusk of evening, but continued to exhibit itself with little-diminished brilliancy for nearly half an hour, finally disappearing with the twilight.

ROBERT DWARRIS GIBNEY

Glan-y-dwr, Crickhowell, South Wales, April 21

WHAT your correspondent, "J. W. B.," saw after sunset was not the zodiacal light, which is easily distinguishable by its great extent of area, lenticular shape, and invisibility during strong twilight, but it may be not incorrectly termed a sun column. I find the following entry in an old journal, of a similar appearance:—"1868, April 17.—Sun column, continuing half an hour after sunset, which was perfectly bright, without clouds." Perhaps some of your readers may be able to explain the cause of it. E. BROWN

Further Barton, Cirencester, April 21

THE phenomenon observed on the evening of Friday, the 6th inst., in Bath, by your correspondent J. W. B. (vol. xxvii. p. 580) was seen at Dolegelly by the writer when on a tour through Wales. On his pointing it out to a companion and some of the townsfolk, all agreed it was quite unique in their experience.

A bright, slender pillar of light, hazy toward the edges, rose majestically from the western horizon, in a cloudless sky, and so continued for about three-quarters of an hour after the sun had set. To one long habituated in meteorological observation it was of a character differing *toto calo* from the path of sunbeams through a cloud-rift, which is invariably divergent in appearance, as if from a focus. The "pillar" was uniform in width, perfectly vertical, and straight, the centre line alone brilliant. The height was, however, greater than your correspondent indicates.

Having fortunately with me a pocket-compass, with plumb-bob for "dip" measurements, I determined (1) the light-pillar was exactly vertical; (2) the height, which scarcely varied during visibility, was 20°, dying out faintly at that elevation; (3) the azimuth 25° north of west. By terrestrial bearings there was an appearance or a slight movement northward, but smallness of the compass dial (1 inch diameter) precluded any reliable angular determination of azimuthal change.

Further, the evening was very cold, and a continuous easterly wind had during the day obscured the hills, which still showed many unmelting snowdrifts upon their summits and flanks. First observed at 7 p.m., the strange appearance faded out at 7.30 p.m.

From the verticality, linear form, and condition of atmosphere I was led to remark at the time to my companion that the phenomenon appeared more of the nature of parhelia than referable to the zodiacal light. An intensely cold easterly wind encountering ocean-warmed airs to the westward would not improbably lead to the ice-molecule condition of atmosphere now assumed to be associated with the occurrence of parhelia.

It may be added (though of little probable significance) that the time corresponded roughly with the time of high water along that coast.

D. J. ROWAN

Kingstown, April 24

### On the Value of the "Neoarctic" as one of the Primary Zoological Regions

PERMIT me to make a few remarks relative to Mr. Wallace's criticisms (NATURE, vol. xxvii. p. 482) of my paper on "The Value of the Neoarctic as one of the Primary Zoological Regions." Briefly stated, it is maintained in the early portion of this paper (1) that the Neoarctic and Palearctic faunas taken individually exhibit, in comparison with the other regional faunas (at least the Neotropical, Ethiopian, and Australian), a marked absence of *positive* distinguishing characters, a deficiency which in the mammalia extends to families, genera, and species, and one which, in the case of the Neoarctic region, also equally (or nearly so) distinguishes the reptilian and amphibian faunas; (2) that this deficiency is principally due to the circumstance that many groups of animals which would otherwise be peculiar to, or very characteristic of, one or other of the regions, are prevented from being such by reason of their being held in common by the two regions; and (3) that the Neoarctic and Palearctic faunas taken collectively are more clearly defined from any or all of the other faunas than either the Neoarctic or Palearctic taken individually.

In reference to these points Mr. Wallace, while not denying the facts, remarks: "The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups and geographical individuality." Numerous families and genera from the classes of mammals and birds are then cited as being entirely wanting in the western hemisphere, and which—in many cases almost sufficient to "characterise the Old World as compared with the New"—"must surely be allowed to have great weight in determining this question." No one can deny that the absence from a given region of certain widespread groups of animals is a factor of very considerable importance in determining the zoological relationship of that region, and one that is not likely to be overlooked by any fair-minded investigator of the subject. But the value of this *negative* character afforded by the absence of certain animal groups as distinguishing a given fauna, is in great measure proportional to the extent of the positive character—that furnished by the presence of peculiar groups—and indeed may be said to be entirely dependent on it. No region can be said to be satisfactorily distinguished from another without its possessing both positive and negative distinguishing characters. Mr. Wallace has in his several publications laid considerable stress upon the negative features of the Neoarctic fauna as separating it from the Palearctic or from any other, but he has not, it appears to me, sufficiently emphasised the great lack, *when compared to the other faunas*, of the positive element, the consideration of which is the point aimed at in the first portion of my paper, and which has led to the conclusions already stated, that only by uniting the Neoarctic and Palearctic regions do we produce a collective fauna which is broadly distinguished by both positive and negative characters from that of any other region. If, as Mr. Wallace seems to argue, the absence from North America of the "families of hedgehogs, swine, and dormice, and of the genera *Meles*, *Equus*, *Bos*, *Gazella*, *Mus*, *Cricetus*, *Meriones*, *Dipus*, and *Hystrix*" be sufficient, as far as the mammalian fauna is concerned, to separate that region from the Palearctic, could not on nearly equally strong grounds a separation be effected in the Palearctic region itself? Thus, if were to consider the western division of the Palearctic region, or what corresponds to the continent of Europe of geographers, as constituting an

independent region of its own, it would be distinguished from the remainder of what now belongs to the Palearctic region by negative characters probably fully as important as those indicated by Mr. Wallace as separating the Neoarctic from the Palearctic region. The European mammalian fauna would be wholly deficient, or nearly so, in the genera *Equus*, *Moschus*, *Camelus*, *Poephagus*, *Gazella*, *Oryx*, *Addax*, *Saiga*, *Ovis*, *Lagomys*, *Tamias*, in several of the larger *Felidae*, as the tiger and leopard, and in a host of other forms. A similar selection could be made from the class of birds (among the most striking of these the *Phasianidae* and *Struthionidae*), but it is scarcely necessary in this place to enter upon an enumeration of characteristic forms. Divisions of this kind, to be characterised principally or largely by negative faunal features, could be effected in all the regions, and in some instances with probably more reason than in the case under discussion.

But the question suggests itself, What amount of characters, whether positive or negative, or both, is sufficient to distinguish one regional fauna from another? Mr. Wallace states: "There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions." Is it to be inferred from this quotation that Mr. Wallace recognises no such general equivalence? Is a region holding in its fauna, say, from 15 to 20 per cent. of peculiar or highly characteristic forms to be considered equivalent in value to one where the faunal peculiarity amounts to 60 to 80 per cent? If there be no equivalence of any kind required, why not give to many of the subregions, as now recognised, the full value of region?

Surely, on this method of looking at the question, a province could readily be raised to the rank of a full region. In the matter of geographical individuality little need be said, as the circumstance, whether it be or be not so, that the "temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate," does not affect the present issue, seeing that the peculiarity is greatly increased by uniting the two regions in question; nor does it directly affect the question of the Neoarctic-Palearctic relationship.

The second part of my paper deals with the examination of the reptilian and amphibian faunas, and the general conclusion arrived at is: "that by the community of its mammalian, batrachian, and reptilian characters, the Neoarctic fauna (excluding therefrom the local faunas of the Sonoran and Lower Californian subregions, which are Neotropical) is shown to be of a distinctively Old World type, and to be indissolubly linked to the Palearctic (of which it forms only a lateral extension)." Towards this conclusion, which, it is claimed, is also borne out by the land and freshwater mollusca and the butterflies among insects, I am now happy to add the further testimony of Mr. Wallace (overlooked when preparing my article respecting the *Coleoptera* ("Distribution," "Encycl. Britann." 9th ed. vii. p. 274).

As regards the name "Triarctic," by which I intended to designate the combined Neoarctic and Palearctic regions, and which may or may not be "somewhat awkward," I beg to state that, at the suggestion of Prof. Alfred Newton (who, as he informs me, has arrived from a study of the bird faunas at conclusions approximately identical with my own), it has been replaced by "Holarctic." In conclusion, I would say that, while the views enunciated in my paper may not meet with general acceptance at the hands of naturalists, it is to be hoped that they will not be rejected because they may "open up questions as regards the remaining regions which it will not be easy to set at rest."

ANGELO HEILPRIN

Academy of Natural Sciences, Philadelphia, April 6

### Mock Moons

A LITTLE before midnight on Monday, the 16th inst., the moon, being nine days old and about 30° above the western horizon, was surrounded by an unusual halo. Its radius was certainly more than the normal 22°. By careful estimation I judged it to be about 30°, the lower edge resting on the horizon. On the right and left limbs of the ring were very distinct bright patches, rather broader than the ring itself, and slightly elongated outwards. The right-hand patch appeared to be in its normal position on a line passing through the moon, parallel with the horizon, but the left-hand patch was distinctly elevated

<sup>1</sup> In the paper under consideration I have given what appear to me satisfactory reasons for detaching certain portions of the South-western United States from the Neoarctic (my Triarctic), and uniting them with the Neotropical region.